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CONTENTS.

The Fundamental Problems of Present-day Plant Morphology: PROFESSOR K. GOEBEL. 33

Scientific Books:—
Clements's Research Methods in Ecology: PROFESSOR CONWAY MACMILLAN. *Strutt on the Becquerel Rays:* G. B. OBEAR..... 45

Scientific Journals and Articles..... 48

Societies and Academies:—
The Missouri Society of Teachers of Mathematics: PROFESSOR L. D. AMES. *The Torrey Botanical Club:* EDWARD W. BERRY. *The University of Colorado Scientific Society:* PROFESSOR FRANCIS RAMALEY..... 48

Special Articles:—
New Work on Wheat Rust: PROFESSOR HENRY L. BOLLEY. *Concerning the Identity of the Fungi causing an Anthracnose of the Sweet-pea and the Bitter-rot of the Apple:* PROFESSOR JOHN L. SHELDON. *Indications of an Entomophilous Habit in Tertiary Species of Quercus:* DR. C. J. MAURY. *Bathygnathus Borealis, Leidy, and the Permian of Prince Edwards Island:* PROFESSOR E. C. CASE. *A System for Filing Pamphlets:* DR. WITMER STONE.... 50

Current Notes on Meteorology:—
Barometer and Weather; Monthly Weather Review, Annual Summary; Climate of Jerusalem; Marine Meteorological Service of Chile: PROFESSOR R. DEC. WARD..... 54

Notes on Forestry:—
Why Prairies are Treeless; Principles involved in Determining Forest Types..... 55

The Fossil Arachnida of Bohemia: H. F. O... 57

Extended Explorations of the Atmosphere by the Blue Hill Observatory..... 57

Regulations governing the Sixth International Congress of Applied Chemistry.. 58

Arthur Woodberry Edson..... 61

George Homans Eldridge..... 62

Scientific Notes and News..... 62

University and Educational News..... 64

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THE FUNDAMENTAL PROBLEMS OF PRESENT DAY PLANT MORPHOLOGY.[1]

A FEW months ago I was in Jena in order to attend the unveiling of the statue there erected to M. Schleiden. Now there is hardly any other place which has been of so much significance in the development of plant morphology as this small university town. It was there that Goethe, the originator of the term 'morphology,' busied himself with morphological studies, and founded the idealistic system which has influenced our thought—often unsuspectedly—till the present day. There Schleiden, in outspoken opposition to the conceptions of the idealistic morphology, gave new life to the theory of development founded by Caspar Frederick Wolff in a neighboring hall in the middle of the eighteenth century, and so paved the way for the brilliant discoveries of William Hofmeister. And who does not know what meaning Jena has won as the citadel of phylogenetic morphology, first through the work of Haeckel in zoology and later through that of Strasburger in botany? In such a morphological atmosphere the question forces itself upon us, in what relation do the morphological questions of the present stand to those of the past? Are they still unchanged in spite of the immense increase of empirical ma-

[1] Lecture delivered at the Congress of Arts and Science in St. Louis, September 21, 1904, by Professor K. Goebel, University of Munich; translated by Professor F. E. Lloyd. The theme was proposed by the Direction of the Congress. Since the time allowed for the lecture was but forty-five minutes, the various questions could be indicated merely.

terial, and have the methods of their solution only changed? Or have the problems themselves become different?

To reply to this question is not easy, and the answer must vary with the point of view of the one who makes it. For morphology is yet far from being an exact science, the results of which force themselves upon us with the compulsion of necessity. This is due to the difficulty of the materials, a difficulty which compels us to seek for hypotheses and other subjective means of explanation. It thus comes about that views not only concerning the goal of morphology, but also as to the way in which this goal is to be reached, are widely diverse, and my own views concerning the fundamental problems of morphology are certainly far from being approved by all morphologists.

We may, indeed, say that, apart from minor differences, there are in morphology two main trends of thought which, apparently at least, are opposed to each other, one of which we may denominate formal, and the other causal. Causal morphology is that the aim of which is to determine the causes, in the widest sense, of form relations; this kind of morphology is the youngest, and is far less widely diffused than the formal. To us of a later period it may seem like a remarkable pleonasm, to speak of a 'formal morphology.' Morphology is, of course, the doctrine of form, and therefore any morphology appears to be, in the nature of the case, a formal one, and as a matter of fact has been in its historical development. But in spite of this fact this definition is historically justified, for it designates the tendency of morphology which regards form as something which stands alone for itself, and takes cognizance neither of the functions of organs nor of how they have arisen. This formal morphology arose at first out of the necessities of taxonomy. There had first to be contrived a terminology for the distinction and description of single plant forms. From this function morphology soon, however, became distinct, thus constituting an independent discipline which on its part had served taxonomy a more important service than one might have at first expected. For while taxonomy, in order to find its way amid the maze of plant forms, had to keep in view the differential characters and the separation of single forms from each other, morphology found itself under the necessity of determining what was common to the most various forms and was accordingly directed toward more general questions; morphology taught, as Goethe expressed it, 'Die Glieder der Pflanzen im Zusammenhänge zu betrachten, und so das Ganze in der Anschauung gewissermassen zu beherrschen.' It resulted in the knowledge that, when we regard plants singly, manifold as their parts appear, they may yet be referred to a few elementary forms, and further, morphological research showed that the parallelism between different plant forms could be understood most easily under the assumption which we designate the theory of descent. The establishment of the theory of descent was the result of the morphological research. This we must here especially emphasize, for it shows what significance morphology has gained in respect to our general conception of organisms. But the theory of descent has also reacted upon morphological research, to such an extent, indeed, that it has been held that phylogenetic research is to be regarded as the sole business of morphology. Thus, for example, Scott has said:

> The object of modern morphological botany is the accurate comparison of plants, both living and extinct, with the object of tracing their real relationships with one another, and thus of ultimately constructing a genealogical tree of the vegetable kingdom. The problem is thus a purely historical

one, and is perfectly distinct from any of the questions with which physiology has to do.[2]

This position is certainly justified from the standpoint of the paleontologist. For him, for whom nothing but dead material is at hand, there remains nothing else to do than to make known, through careful comparative study, the structure and relationships of those organisms whose remains are available. This is a very important business. The beautiful results of phytopaleontological research, such as have been attained during the last decade in England and France, have very materially furthered our knowledge of plant forms, and have made to live again before our eyes in a most surprising manner and in the finest details of their structure, types long since vanished from the surface of the earth.

But does this limitation of morphology to the comparative phylogenetic method which is imposed upon the paleontologist exist also for the morphological study of living plants?

There are many of the opinion of Scott; and, indeed, a special 'phylogenetic method,' which is said to be a characteristic of modern morphology, has even been talked of.

Were this the case, then the only difference between the morphology of the present and the earlier, idealistic morphology would consist in this, that in the place of the general ideas with which this operates, as, e. g., 'type,' 'plan of organization,' etc., there would be found phylogenetic conceptions. Such general abstractions are, however, even now difficult to escape, since we can set forth real descent-series only in the fewest instances, and, accordingly, we can not actually point out the stem forms. Yet Darwin himself said:

> We have seen that the members of the same class, independently of their habits of life, resemble each other in the general plan of their organization. This resemblance is often expressed by the term 'unity of type'; or by saying that the several parts and organs in the different species of the class are homologous. The whole subject is included under the general term of Morphology. This is one of the most interesting departments of natural history, and may almost be said to be its very soul.[3]

The significance of formal morphology can not be more forcibly expressed than it was by Darwin. And yet we see that, in Germany at least, interest in morphological problems has greatly decreased. Morphological treatises have become relatively less numerous; morphological books, even such excellent ones as, *e. g.*, Eichler's 'Blüthendiagramme,' do not pass through a second edition, while anatomical and physiological works appear repeatedly in new editions; evidently meeting the demands of the botanical public more fully than morphological works. This may be referred to reasons which lie partly without and partly within morphology itself; both turn out to be true. Histology, cytology and experimental physiology have developed remarkably; new methods in this field promise new results; particular lines of work, however, such as descriptive anatomy, are especially favored because the perfection of the methods of research have quite materially lightened the task of working through a vast array of materials, especially for those to whom the other fields of botanical study are more or less unfamiliar.

But the reasons for the phenomenon which lie within the field of morphology are also clear. Some parts of morphology are well worked out, as, *e. g.*, the doctrine of the more obvious form relations of plants, and the homologies, at least in the large, are determined, although in the matter of detail much remains vague and offers a wide field for exhaustive studies in development. More and more, however, these

[2] Address to the botanical section, British Association for the Advancement of Science, Liverpool, 1896.

[3] 'Origin of Species,' 2: 142.

studies bear the stamp of repetition and complement, from which the stimulus of newness is wanting, or they are carried on upon materials which are very difficult to obtain. The constructions of the idealistic morphology, however, often proved to be untenable.

But the first experiments towards a causal morphology, brought disillusion. For only a short time lived the hope of being able to answer, *e. g.*, the question as to the arrangement of leaves through the effect of mechanical factors, or to refer the form-relations of a plant to the direct influences of gravity and light on the plant. It soon became evident, however, that such involved problems are not to be unraveled by such simple means, and this may well have resulted in the suppression of interest in morphology.

At this point phylogenetic morphology appeared to take on a new lease of life. This, however, in natural science is connected, on the one hand, with the appearance of a new, creative (?) idea, and, on the other hand, with the discovery of new methods. Now the theory of descent has powerfully stimulated morphological research. But has it brought to it, as, *e. g.*, Strasburger has held, a new method, the phylogenetic? Alexander Braun has already properly answered this question in the negative.

Scott, also, has maintained that historical morphology (as regards both living and fossil plants) is dependent upon comparative study, that is, makes use of the same method as was in evidence before the appearance of the theory of descent; indeed, the most important homologies in the plant kingdom became known through Hofmeister at a time when the idea of descent was far from that general acceptation which it at first gained through the life work of Darwin.

The method has then from first to last remained the same: the most comprehensive comparison not only of mature forms, but also their development. A special 'phylogenetic method' there is not, but only a phylogenetic conception of morphological problems. These are, however, just as at first was the case with idealistic morphology, purely formal. Modern morphology, in my sense, however, differs from the older in this, that it goes beyond the method of mere comparison. It allows the setting up of genetic trees to rest for the while, since, with our present knowledge, this meets with insuperable difficulties and has brought almost as much disappointment as the idealistic morphology. For just this reason, namely, because we are persuaded that no other forces have been at work during the phylogenetic history than those which now control the development of each particular organism, do we wish, first of all, more exactly to learn what these are. We are concerned not alone with the determination of the single successive stages of development. These must, of course, be followed, but in addition we should follow all phenomena which may be got at by our means of observation, whether directly, by the microscope, or by chemical analysis. We may, therefore, say: The basal problem of the present day morphology is not phylogenetic development, but development in general. We must, therefore, take our departure from the investigation of individual development (of ontogeny), for only this lies before us complete and without any break, and further, because the study of ontogeny only may proceed from the experimental point of view. An understanding of development is possible only when the conclusions, to which the observation of the phenomena of development has led us, rest upon experimental proof; in other words, when we ask questions of Nature, and obtain our answers to them.

Every little step—and with such only

are we now concerned—beyond the mere descriptive consideration of development is here of significance, and brings the possibility of further progress. And small indeed, I may add, appears to be such advance to those who, from the beginnings of phylogenetic morphology have, like Sisyphus, sustained their courage to roll again and again up the mountain the rock of phylogeny as often as it has rolled down.

It may now be attempted to examine somewhat more closely in certain particular examples the relation between phylogenetic and causal morphology. One of the changes which phylogenetic morphology has brought with it is that it seeks to ascertain which form is 'primitive' and which derived. Idealistic morphology has borne in upon us no conviction on this question, since it derives all forms from a type which is present only as a conception. But phylogenetic morphology must, on the one hand, always reckon with the possibility of polyphyletic development, and, on the other hand, it can operate not only with reversionary structures, as did the idealistic morphology, but must be far more concerned in determining which forms within the series which it proposes stand nearest the common point of derivation. It seeks then with diligence after 'primitive' forms. But in this search we meet with great difficulties. In the first place, we are inclined to regard those forms as primitive which have simple form-relations, and unmarked division of labor. But such forms may also have arisen by reversion, and if one looks over botanical literature, he sees, at least so far as the relationships between the larger groups are concerned, there exists no agreement as to which forms are to be regarded as primitive and which derived; often opinion on this point changes with the fashion. Thus the thallose liverworts have up till now been regarded as more primitive than the foliose, because the vegetative body of the former is much more simple in construction than that of the latter, and between them there are found gentle gradations. Recently, however, the attempt has been made to derive the thallose from the foliose forms. This is not the place to examine the evidence for or against such derivation. How vacillating is the point of view from which it is judged what form is primitive is shown by the various positions which have from time to time been given to the apetalous dicotyledons.

The old morphology regarded these as reduced forms because their flowers are less fully differentiated than those of most of the other dicotyledons. Eichler has, however, already shown that there is no ground for maintaining that the corolla in the 'Iuliflorae' and 'Centrospermae' has suffered reduction; and on this point we can only agree with him. But must they, because the perianth shows simpler form relations and also because the number relations within the flower are not always constant, be therefore primitive? Even if we admit that these groups have a great geological age, it is not proved that they stand as regards their total organization on a lower plane of development; old and primitive forms are the same only when it can be shown that the former stand nearer to the stem forms of the angiosperms than other forms. If this is not capable of proof, then the old forms may just as well be the end terms of long developmental series as others, only that the differentiation of organs has not taken place to the same degree as in the others. Now, we do not know the stem forms if the angiosperms, and they may never, perhaps, be known. But even if we content ourselves by reconstructing them on the basis of comparative study, I can find no reason, *e. g.*, to regard the Cupuliferae as primitive forms, while I can find many reasons for not doing so.

Here may be cited chalazogamy, which elsewhere occurs in forms which may be regarded as degenerate; the facts that only a few of the ovules develop further; that at the time of anthesis they are in many forms not yet present, and finally the dicliny of the flowers. There has been much contention over the question whether the androgynous flowers of these forms are to be admitted to be the original form or not. Let us look at, *e. g.*, the Cupuliferæ. Most of the forms have diclinous flowers. In *Castanea vesca*, however, androgynous flowers occur regularly, and in the male flowers rudiments of the ovary, in the female flowers staminodia are often evident. But we know that for reduced organs all gradations occur from nearly complete development to almost entire disappearance. From the formal standpoint, then, the androgynous flowers may, with at least as much justice, be regarded as primitive as the diclinous ones, which, more recently, have been thus branded. Just this question is, however, fitted to clear up the difference between pure phylogenetic and causal morphology. The latter says: By the mere comparison of forms morphological questions may not at all be decided. We must first of all become more closely acquainted with the forms to be compared, by seeking to determine the conditions under which, in living plants, the configuration of parts is produced. Concerning the flowers of the Cupuliferæ the question then arises: is the occurrence of male and female flowers dependent upon different conditions and are these other than those under which androgynous flowers arise? As a matter of fact, it may be determined that, *e. g.*, in the oak the female flowers always occur in those parts of the twig which are stronger, that is, better nourished than those in which the male flowers occur. This offers us, however, only a point of departure for a more exhaustive research. When we know better the relation between the formation of flowers and the total activity of the plant, when we have the ability at will to cause it to produce male, female or androgynous flowers, when we further know how it is determined that the oak usually brings to development only one out of six ovules, and why the pollen tube follows a different path than the usual, then may we further discuss the question whether the Cupuliferæ are primitive or not—for then shall we have better grounds for phylogenetic conclusions than we have at present, and we shall then recognize with great probability the changes which have taken place in these organs as phenomena resulting from changes in the total organization of these plants.

So, as the matter now stands, we can not deceive ourselves on this point, that the constructions of the old morphology, although confined almost entirely to *vestigial* series, nevertheless stood on firmer ground than the modern speculations on the question of primitive forms. Starting with a completely endowed form, we can follow the reduction of form through intergradations and, by reference to vestigial organs, often with convincing certainty. But by what means shall we judge a rudimentary organ? Is it more than a gratuitous assumption, when, as recently was the case, a certain botanist declares the lodicules of grasses to be not a perigone, but a rudiment (*Ansatz*) of a perigone? Whereby may one recognize a rudiment, *i. e.*, the attempt to form something new, an attempt which, however, has remained nothing more? In what way may we distinguish such a rudiment from a vestigial organ? And, finally, after one has broken faith with the old vestigial series, is it not still more of the stamp of formal morphology if he contents himself in arranging forms in series and then comes to a standstill when he tries to decide at which end

stand the primitive and at which end the derived forms? At any rate, such a limitation brings out the better the true condition of our knowledge, for such an arrangement of forms in a series is about the best service that formal morphology can do. This service is, to be sure, no small one, for it enhances broad critical comparison and is, therefore, the result of hard work. But the desire to give this arrangement in series a genetic bearing has oftentimes led us to untenable propositions and explanations. Just as we have little ground for assigning the Cupuliferæ to a primitive position, so have we as little evidence for regarding the Casuarinæ also in the same light. The latter have been placed by a recent systematist at the apex of his system because there has been an inclination to find in them a sort of 'missing link' between angiosperms and gymnosperms. I may, perhaps, mention that I had regarded such a view as incorrect, even before the evidence was adduced by an American botanist (Frye) that *Casuarina* has evidently nothing which marks it off from other angiosperms. Many of my fellow botanists have been inclined to point out as a further example of the fruitlessness of the search for primitive forms those Bryophytes which have been regarded by me as primitive; and I readily admit that here also we can not point out any conclusive evidence for their primitive position, but only for a greater or less subjective probability. Numerous other examples (as, *e. g.*, the supposed primitive monocotyledons) may be pointed out, which show that the phylogenetic morphology has overrated the prospects of results in search for primitive forms, stimulating as this has been.

This may be seen also if we notice the attitude of phylogenetic morphology to the problem, which the old morphology dubbed with the not very fortunately chosen name of metamorphosis, and which historically is that of homologies. Here, also, it may be shown that the problems have remained the same while only the attempts to reach a solution have changed.

The idealistic morphology believes that all organs of the higher plants may be traced back to caulome, phyllome and trichome; it conceived this process not as a real one, but was content with a conceptual arrangement of different plant organs in these categories, which were really nothing but abstractions.

That thereby the reproductive organs were left entirely out of consideration—these were referred to modifications of vegetative organs—is explained in part by the fact that they occur in the higher plants less frequently as peculiar parts, and often completely disappear in teratological growths, which are with predilection turned to account in theoretical considerations; and in part because of the view that for morphology the function of an organ is a matter of indifference, and that accordingly in morphological considerations it can have no significance whether an organ has developed as a glandular hair, chaffy scale or as an archegonium, so long as it has developed out of the outer cell-layer of the plant body! This standpoint, a complex one, indeed, needs no especial discussion more. Let us, on the other hand, see how phylogenetic morphology has come to terms with the problem of metamorphosis. As an example I select a passage from a prominent American work, in which Coulter and Chamberlain express themselves concerning the leaf structures of flowers as follows:

> While sepals and petals may be regarded as often leaves more or less modified to serve as floral envelopes, and are not so different from leaves in structure and function as to deserve a separate morphological category, the same claim can not be made for stamens and carpels. They are very ancient structures of uncertain origin, for it is quite as likely that leaves are transformed

sporophylls as that sporophylls are transformed leaves. * * * To call a stamen a modified leaf is no more sound morphology than to call a sporangium derived from a single superficial cell a modified trichome. The cases of 'reversion' cited are easily regarded as cases of replacement. Lateral members frequently replace one another, but this does not mean that one is a transformation of the other.'

We see that in this verdict the emphasis is laid on the historical development, but at the same time this is pointed out to be unknown to us. With this latter conclusion I am in complete harmony, but the accentuation of the historical-phylogenetic factor has, on the other hand, led to a conception of the ontogenetic problem, in which I can perceive no advance upon the old morphology; there is rather avoidance of the problem than an attempt to solve it. This, however, is connected with the purely formal conception, as the phylogenetic morphology employs it. Let us examine the matter in question. For a long time we have known that often in the room of the stamens—to confine ourselves to these—flower leaves or foliage leaves or occasionally even carpels arise. The idealistic morphology says that this proves that the stamens are 'leaves,' for these can be modified the one into the other. Coulter and Chamberlain, however, deny that a stamen fundament may be transformed into a flower leaf; they find only a 'replacement' of one 'lateral member' by another. It should be remarked that 'leaves' exist in nature as little as 'lateral members.' Both notions are mere mental abstractions, not the expression of the facts of observation. We speak of the replacement of one organ by another if these have nothing more in common than the place of origin. Thus we see that in the foliose liverworts a branch often arises in the position of a leaf-lobe. No one has observed any intermediate form between these; the lateral shoot in reality takes only the position of a leaf-lobe. The relation between the stamens and the organs which 'replace' them is, however, quite different. We speak of a transformation of an organ *A* into an organ *B* when *B* not only stands in the position of *A*, but also corresponds with *A* in the earlier stages of its development, and later strikes out on its own line of development. If this is the case, we should expect to find between *A* and *B* intermediate forms which are different according to the developmental stage at which *A* is caused to develop further as *B*. To use an analogy: Replacement and transformation behave as two fluids which are, and two fluids which are not miscible; in the first case the inner structure is different, and in the second there is a correspondence. The comparison is a limping one, but still gives us a fair illustration.

As a matter of fact, we do find every intermediate step between stamens and flower leaves, and we can not doubt that these have come into existence because a stamen, or, in other words, a stamen fundament, has at different stages of its development received a stimulus which has caused it to develop into a flower leaf. We find correspondingly, that the earlier developmental stages of a stamen and a flower leaf are parallel throughout, while in the above cited example of the branch and a leaf-lobe of a Jungermanniaceous liverwort the developmental history are throughout different, as is shown by the arrangement of cells. In the case of stamens, therefore, there occurs not a replacement, but a transformation. And, indeed, a limited one. Not any 'lateral members' you please may arise instead of stamens, but only and always those which we subsume under the concept leaf, because they evidently have peculiarities in common. Besides, there are also normal flowers which exhibit all intergradations between flower

[1] Coulter and Chamberlain, 'Morphology of the Angiosperms,' p. 22.

leaves and stamens. The former Coulter and Chamberlain would regard as leaves, the latter not; where, however, is the line of separation between them?

From the limited power of transformation possessed by organs it results that in causal morphology the problem is then not a phylogenetic, but an ontogenetic one. Whether sporophylls or foliage leaves are the older phylogenetically may be disregarded. For it appears more important first to determine why the power of transformation is limited, why a shoot-thorn or a shoot-tendril may be transformed only into a shoot, a stamen or a carpel only into a 'leaf'; and second, what conditions are determinative thereto.

The first step toward the solution of the problem is that we learn to call out experimentally and at will such transformations as we have heretofore occasionally observed as 'abnormalities.'

This has been successful in experimental morphology in a great number of cases, and in the future will be still more so. To be sure, we are still unable to induce the transformation of stamens into flower leaves at will—we only deceive ourselves when we believe that the art of the plant breeder has succeeded in doing this, for in reality all he has done is to isolate such races which have occurred in nature with more or less doubled flowers—and in this regard we stand in contrast to the fungi and insects, the activities of which, as Peyritsch and others have shown, often—unconsciously of course—call forth such transformations. Yet it has been possible to change scale leaves (cataphylls) and sporophylls into foliage leaves, inflorescences into vegetative shoots and, *vice versa,* plagiotropous into orthotropous shoots, hypogæous into epigæous, not to mention the interesting results which have been obtained by Klebs in his studies of the lower plants.

Let us take, for example, the just mentioned transformations of scale leaves into foliage leaves and of sporophylls into sterile leaves. Here developmental study and experiment immediately encroach on each other. Development has shown that, *e. g.,* the bud-scales of many trees which in their definitive condition are very different from the foliage leaves, yet parallel them developmentally in an extraordinary degree; and that many bud scales possess the fundament of a leaf blade which has failed to develop and has thus become vestigial. Similarly, the fundaments of the foliage leaf and the sporophyll in *Onoclea* are the same up to a quite late stage of development, beyond which each follows its own course. These facts gave occasion to the question whether or not it were possible to influence the development at will, and so to cause a scale leaf or a sporophyll to grow from a fundament which otherwise would develop into a foliage leaf. It has been shown that such transformations may be occasioned in a simple way, and the developmental correspondence makes such a limited transformation without further difficulty capable of being understood. And since seedlings produce, apart from the cotyledons and certain adaptations in hypogæous germination, only foliage leaves, which are arranged for the work of photosynthesis; since further it is seen that all foliage leaves of one and the same plant, different as they appear externally, yet in reality follow one and the same course of development, which, as we have seen, is remarked also in scale leaves and sporophylls; I accordingly come to the view that other leaf organs are derived from foliage leaf fundaments through a change in the course of development occurring at an earlier or later period of growth. This conception has found many opponents, some of them for the reason that they have not been able to free themselves from the purely historical conception of the problem.

But the historical question can not help us over the ontogenetic problem any more than the solution of the latter alone can answer the historical question. Even if it were proved in all cases that sporophylls, flower leaves, sepals, etc., are transformed foliage leaves, it would remain undecided that these are phylogenetically older than the former. This phylogenetic problem, however, is with our present means and knowledge not subject to solution with certainty, while the ontogenetic problem, on the contrary, is. Problems, however, which may not be solved appear to me less important than those which may.

To be sure, the solution of the ontogenetic problem is hedged about with great difficulties. For the results which have already accrued, valuable as they may be, take their importance from the fact that they lay the foundation for the future work: what changes take place during transformation, and upon what outer and inner conditions are they dependent? We may comfort ourselves as little as could Goethe at one time with the view that flowers differ from the vegetative shoot in a refinement of the sap; rather would we know what change of the materials, and what other changes, are connected with the order of successive developmental stages of the flower. This, to us as good as unacquired knowledge, should give us a more penetrating glance into the nature of development than we have as yet had. To just this purpose plants are especially well adapted, for experience has shown us that the development of a plant is not produced as is the melody in a music box, in a definite order so long as the outer source of power is present to start it; for the experiments of the last few years indicate rather 'that the form relations of chlorophyll-bearing plants are not predetermined in the germ cell, but in the course of development.'[5] As a result we can not only arrest development at any particular stage, but we can also cause fundaments to unfold which were previously 'latent.' Historical morphology has contented itself as regards the unfolding of latent fundaments also with an historical explanation of the facts. The observation, *e. g.*, that instead of the seed scale of the Abietineae under certain circumstances an axillary shoot appears, has been used by prominent botanists to support the conclusion that the seed scale has arisen phylogenetically from a shoot. Such an hypothesis would get beyond the rank of pure supposition if a living or fossil form certainly related to the Abietineæ could be pointed out, the cones of which bear in the axils of the cover scales shoots possessed of macrosporophylls. As long as such proof is not forthcoming, we stand opposed to a phylogenetic explanation of this observation 'kuehl bis ans Herz hinan.' We seek rather to establish the conditions under which the fundaments, which otherwise become seed scales, develop into shoots, and hold before us therewith the possibility that the forebears of the Abietineæ could have borne their ovules upon axillary outgrowth of the cover scales, which, indeed, possessed the ability under certain circumstances which disturbed the normal development to form shoots, but which phylogenetically does not need to have been at any time an axillary shoot.

The question of the significance of metamorphosis leads us into another field of morphology. The above-cited examples show that the transformation of organs always goes on hand in hand with a change of function. This gives us the occasion to take up a further problem of modern morphology: the relation between form and function. The old morphology believed that it should keep away from this question because it held that the function of an organ had nothing to do with its 'morphological meaning.' Just recently we

[5] Goebel, 'Flora,' 1895, p. 115.

have heard that morphology has to do with 'members' and not with the 'organs' of a plant. The fact that 'members' and 'organs' mean one and the same thing, and that for the organism their members are organs, or tools, shows that here again is a purely artificial and therefore untenable abstraction. Morphology stiffens to a dead schematism when it does not take the plant for what it really is—a living body the functions of which are carried on in intimate relation to the outside world. It was the powerful influence of Darwinism that turned more attention again to the function of single plant organs, for, according to one view, which has many adherents, all form relations arise through adaptation. D. H. Scott has given clear expression to this view in the sentence, 'all the characters which the morphologist has to compare are, or have been, adaptive.'

This is a widely disseminated conception, but is by no means as widely accepted. Above all, it must be pointed out that it is not the result of observation, but is a theory, which enjoys by no means general acquiescence. True, the conclusion drawn depends upon the meaning given to the word 'adaptive.' But take it as you will, in the Lamarckian or in the Darwinian sense, in reviewing the phenomena of adaptation we come face to face with the problem: are the form characters fixed adaptational characters solely, or have we to distinguish between organization and adaptational characters? There are several grounds which have led to the belief that organization and adaptational characters coincide. Chiefly the brilliant results which investigation concerning the functional significance of structures as well in the flower as in the vegetation organs has had in the last decade. It was evident that structures to which were earlier ascribed no sort of function yet have such. And if none was found, there yet remained the possibility that the structures concerned had earlier been useful as adaptations. It is, however, clear that we are hereby near to the danger of accepting something as proved which needs rather to be proved. In reality, it seems to me that morphological comparison as well as experiment shows that the distinction between organization and adaptational characters is justified, and that the opinion to which Scott has given expression has arisen from the admission that specific characters have arisen through the accumulation of useful fluctuating variations effected by the survival of the fittest. But we see that in many cases specific characters are not adaptive. If we follow out, *e. g.*, the systematic arrangement of the Liliifloræ, we see that the particular groups differ from each other as to whether the ovary is inferior or superior, and whether it later becomes a capsule or a berry, and, if it is a capsule, whether it is loculicidal or septicidal. Concerning these characters one may well ask whether one can bring the berry or the capsule into relation with the question of adaptation; whether it can be shown that the berry-bearing Liliifloræ occur or have arisen chiefly in those regions where also occur many birds which devour the berries and thus disseminate the seeds. Such a relation can not at present be shown to exist. And who would regard the question whether a capsule opens septicidally, as in the Colchicaceæ, or loculicidally as in the Liliaceæ, as one which stands in relation to adaptation? The method of opening is conditioned by the structure of the fruit in the Colchicaceæ and Liliaceæ, but for the scattering of the seed it is evidently quite a matter of indifference. Shall we conclude that in the past it was otherwise?

Here again we are shown that we get along the best when we start out with the observation of the plants which surround

us, and not with theoretical assumptions and far-reaching phylogenetic hypotheses. The theory of mutations formulated by de Vries with such brilliant results is the result of this kind of patient and step-by-step observation of the now living plant world. The observations of de Vries show us that specific characters arise not through the accumulation of useful variations, but by leaps, and have nothing at all to do with direct adaptation. Such as are disadvantageous in the struggle for existence are weeded out. But selection can not effect the origin of specific or organization characters as such, and this makes it clear to us why—from the human standpoint—one and the same problem may be solved in such different fashions.

The mutation theory of de Vries limits itself to that alone which the observation of the present moment can come at, to the origin of the so-called 'minor species.' But how the division of the plant kingdom into the larger groups has come about, how it has happened that the 'archetypes' have reached such marked development and others have died out or remained in abeyance, are further problems, the solution of which may not so soon be looked for. For this, however, the more intimate knowledge of the factors which regulate the development of the individual from the egg cell to the ripening of the fruit, forms a fundamental starting point. For this purpose plants are especially suitable, since, on the one hand, because of the possession of a *punctum vegetationis*, they are in later life also provided with embryonal tissue, and, on the other hand, because in their form they are more exposed to the influence of the outside world than the majority of animals.

An especially important means in order to the causal study of development has the research into those phenomena proved itself, which we designate the regeneration of new formations as the result of wounding. The questions: what really takes place when an embryonic cell becomes a permanent cell; the reciprocal influences of separate plant organs, which we call correlation; further the problem of polarity; stand out with great clearness in the phenomena of regeneration. I can, however, at this moment only indicate the problems, and can not point out the steps which have been taken toward their solution. A wide vista spreads out before us. The more must we wonder that of the countless botanical papers which appear each year not more than perhaps a dozen are concerned with the problem of development.

Summing up this brief presentation, it should have been shown that morphology, which originally formed a part of taxonomy, then grew apart from it as an independent discipline. Only when it gives up this separate position will morphology take on new life, for such a position is warranted only historically and not in the facts.

The earlier morphologists would have said that morphology has as little to do with the physiology as with the anatomy of plants, which latter, at the time when systematic botany was in the ascendant, they reckoned also as physiology. For physiology was then everything which was not taxonomy. Nowadays it would be carrying coals to Newcastle to point out the significance of the cell doctrine for morphology. For the understanding of alternation of generations, of inheritance and other phenomena fundamentally important to morphology the doctrine of the cell has become of basic significance. The same is true in a higher degree for the relation between morphology and physiology, for all other tasks of the descriptive natural sciences are, after all, only preliminary attempts at orientation, which at length lead

to experimental questioning, to physiology. Indeed, one may say that morphology is that which is not yet understood physiologically. The separation of the different tasks of botany is not in the nature of things proper, but is only a preliminary means at first to orientate ourselves with reference to the maze of phenomena. The barriers between these tasks must then in the nature of the case fall with further progress. I do not wish to deny the value of phylogenetic investigation, but the results which it has brought forth resemble more the product of creative poetic imagination than that of exact study, *i. e.*, study capable of proof. If the knowledge of the historical development of plant forms hovers before us as an ideal, we shall approach it only when we attack the old problems of morphology, not simply with the old method, that of comparison, but experimentally, and when we regard as the basal problem of morphology not phylogenetic development, but the essence of development in a large sense. Even if we had the story of development spread out clearly before us, we could not content ourselves with the simple determination of the same; for then we should be constrained to ask ourselves, how it has been brought about. But this question brings us straight back to the present, to the problem of individual development. For there is for natural science hardly a more significant word than this of Goethe's: 'was nicht mehr entsteht, können wir uns als entstehend nicht denken. Das Entstandene begreifen wir nicht.' It is then the task of modern morphology to learn more exactly the factors upon which at this time the origin of structures depends. To this task, for which there was at that time but little preparatory work consisting of a few important attempts by the gifted Thomas Knight, Wilhelm Hofmeister, who is known to most of us only as a comparative morphologist, did a too little recognized service. For he pointed out, even before this trend of study became apparent in zoology, that the ill-designated 'Entwickelungsmechanik' pursues essentially the same goal as the causal morphology of botany.

We may regard as a motto this sentence from Hofmeister's 'allgemeiner Morphologie': 'es ist ein Bedürfnis des menschlichen Geistes, eine Vorstellung sich zu bilden über die Bedingungen der Formgestaltung wachsender Organismen im allgemeinen.' This is even now the problem of present day morphology. Comparative consideration, including, of course, the especially important history of development, offers us valuable preparation for the intellectual grasp of the problem, but, above all, for the pursuit of the experimental method.

That the zoologists also have felt this necessity to strike out into new ways besides that of comparative morphological observation shows anew that for all organisms the problems are really the same. Let us then take for our watchword development, not only as a problem, but also for the methods with which we seek to bring ourselves nearer its solution.

SCIENTIFIC BOOKS.

Research Methods in Ecology. By FREDERIC E. CLEMENTS, Ph.D. Lincoln, Nebraska, The University Publishing Company. 1905. Pp. xvii + 334.

This work by Professor Clements is intended by the author as a handbook for investigators and for advanced students of ecology, and not as a text-book of the subject. It, therefore, contains a somewhat elaborate account of methods used by the author in his studies of the last eight years during which a serious attempt has been made by him to discover and to correlate the fundamental points of view in the vast field of vegetation.

The book is presented in four chapters or

parts, the first of which deals with the scope, historical development, present status and important applications of ecology. Under the latter caption its connection with physiology and pathology, experimental evolution, taxonomy, forestry physiography, soil physics, zoogeography and sociology is pointed out. The second chapter is concerned with the habitat and methods of its investigation. First, the factors are determined and classified and then an account is given of the various instruments, including geotomes, psychrometers, psychrographs, photometers, selagraphs, thermometers, clinometers, trechometers, etc., which are employed in the study of the habitat, together with methods of charting statistics. By these instruments and methods statistical analysis of soil water content, humidity, light, temperature, precipitation, wind, soil structure and chemistry, altitude, slope, exposure, etc., is made possible. There is attention paid to the details of choosing instruments, stations and readings. The plotting of curves and determination of graphic representations is explained. The third chapter has to do with the plant, the stimuli which it receives, the nature of its response, its adjustment and adaptation especially to water and light as stimuli. Here some experimental evolutional methods are set forth having to do with the selection of species, the determination of factors and the recording of habitat cultures and control cultures. The fourth chapter, which is the most extensive and to which the other three are in a sense preliminary, has for its general subject the formation or vegetation unit consisting essentially of plants in a habitat, the need for exact investigation of which is set forth by the author. Quadrats, transects, migration circles, are described and their use explained. The two important arts of cartography and photography in their relation to ecologic inquiry are given space, and methods of preparing and preserving formation and succession herbaria are indicated. The development and structure of vegetation are taken up under the conception that the formation is a complex organism which possesses functions and structure and passes through a cycle of development similar to that of the plant. The functions of a formation are described as association, invasion and succession, while its structure may comprise zones, layers, consocies, societies, etc., all of which may be referred to zonation or alternation. Thus classified, the facts and laws of migration, dissemination, barriers and indemism, polygenesis, etc., are given detailed discussion. The final pages are devoted to experimental vegetation with description of culture methods for formational aggregates.

Throughout the volume Professor Clements makes use of his carefully devised nomenclatural system, the extent of which is very considerable. A great many thousand new words and nomenclatural combinations are proposed, for the most part derivatives from the Greek. This system has already been brought to the attention of ecologists in papers in Engler's 'Jahrbücher' (1902) and elsewhere and has attracted attention. A glossary of ten pages in which the more fundamental new words are gathered is a helpful addition to the work. An index is not provided, but the glossary to some extent fills its place.

Eighty-five illustrations, for the most part half-tones, of vegetation, plants and apparatus serve to lighten up the text. Most of these are original views of Colorado vegetation prepared by the author and give a very excellent idea of the grouping of plants upon some of the mountain habitats.

Altogether, Clements's 'Research Methods in Ecology' is a notable contribution to the literature of ecology and the author is to be congratulated upon its successful delivery to the botanical world. Conway MacMillan.

The Becquerel Rays and the Properties of Radium. By Hon. R. J. Strutt. London, Edward Arnold. 1904.

One closes Mr. Strutt's book with a feeling of rare satisfaction. The reader has had a clear and concise presentation of the fundamental phenomena of the Becquerel rays, and the phenomena centering around that astonishing substance, radium.

The book, which lays no claim to an exhaustive treatment of the subject, is primarily written for those who wish to know something about radioactivity and are interested in sci-

ence. Abstruse ideas are left out just so far as possible, and the work, although not phrased in popular language, as we understand the term to-day, is nevertheless very clearly and readably written. Mr. Strutt has evidently the faculty of presenting scientific facts in a very pleasing and attractive and convincing form. In short, he shows the stamp of the teacher.

Notwithstanding all the evident care of preparation in the work, Mr. Strutt has once or twice allowed himself to become careless in the use of terms. For example, on page 66, when treating the selfelectrification of radium, he uses the term 'perpetual motion,' stating that the above phenomena more clearly approach it than any other action we are acquainted with. It seems to me that the term is very wrongly applied, because perpetual motion means the performance of work without the expenditure of energy; and there certainly is energy expended in the above phenomena. Later on in the book (page 167) he states that 'Nothing that we can do to any portion of matter will in the smallest degree affect its weight,' etc. This is rather too loose a statement to let pass without comment.

Chapter I. discusses some general statements about the discharge of electricity through gases, a subject that Dr. J. J. Thomson has so thoroughly investigated. The discussion of the cathode and Roentgen rays contains descriptions of the various phenomena connected with them.

Chapter II. deals with the discovery of radioactivity, the first portion treating of the signal work of Becquerel, who discovered the rays which bear his name, namely, those rays which are given off by some salts, and which have photographic properties, but which differ from Roentgen rays and also from ordinary light.

The discussion of the various phenomena relative to uranium prepares the reader for the story of the discovery of radium. Mr. Strutt explains in an interesting and instructive manner the chemical procedure gone through to make it possible to obtain radium from pitchblende, a mineral in which was discovered by Madame Curie a radioactivity far in excess of what was expected. This fact led Madame Curie to conclude that there resided in the mineral some other more active constituent than uranium.

In Chapter III. we have the properties and nature of the radiations described together with the fluorescent, chemical and physiological effects. This is followed by a rather complete discussion of the alpha rays, beta rays and gamma rays, with several diagrams illustrating experiments carried out to determine the deflection of the alpha rays and beta rays. The experiments with gamma rays have so far been negative.

At considerable length in one of the chapters is described the subject of radioactvity in the earth and its atmosphere, a branch of the work to which Mr. Strutt has contributed rather extensively. Other chapters treat of absorption and ionization; changes occurring in radioactive bodies; the products of radioactive change.

The last chapter gives a short account of the electrical theory of matter, to which Professor J. J. Thomson has given much thought. This chapter is very suggestive and supplies food for contemplation.

At the end of the book there are three appendices, the first of which contains a number of experiments which any one may perform with a bit of radium. A sufficient amount of radium necessary for these experiments would cost in the neighborhood of twenty-five dollars. With this appendix are given two plates.

The other two appendices treat respectively of the mathematical theory of the deflection of the beta rays; a short account of the extraction of radium from large quantities of pitchblende.

The book is non-mathematical throughout, except for the appendix mentioned, and shows a keen insight into the subject dealt with. This, however, is to be expected from a man who has done so much work in the realm of radioactivity. On the whole, the book is to be commended to those who are of a scientific turn of mind, and who wish to understand clearly the principal phenomena of radioactivity.

G. B. OBEAR.

PROVIDENCE, May 24, 1905.

SCIENTIFIC JOURNALS AND ARTICLES.

THE *American Journal of Science* for July contains the following articles:

D. A. KREIDER: 'Iodine Titration Voltameter.'

F. A. GOOCH: 'Handling of Precipitates for Solution and Reprecipitation.'

R. H. ASHLEY: 'Estimation of Sulphites by Iodine.'

M. TALBOT: 'Revision of the New York Helderbergian Crinoids.'

L. V. PIRSSON: 'Petrographic Province of Central Montana.'

T. HOLM: 'Croomia pauciflora.'

E. RUTHERFORD and B. B. BOLTWOOD: 'Relative Proportion of Radium and Uranium in Radioactive Minerals.'

J. TROWBRIDGE: 'Side Discharge of Electricity.'

H. L. BRONSON: 'Effect of High Temperatures on the Rate of Decay of the Active Deposit from Radium.'

THE contents of the June issue, *Terrestrial Magnetism and Atmospheric Electricity,* are as follows:

Frontispiece: Portrait of Karl Selim Lemström.

H. GERDIEN: 'Die Absolute Messung der specifischen Leitfähigkeit und der Dichte des verticalen Leitungsstromes in der Atmosphäre.'

J. DE MOIDREY, S.J.: 'Mesures magnétiques en Chine.'

H. F. REID: 'Records of Seismographs in North America and the Hawaiian Islands.'

HJ. TALLQUIST: 'Karl Selim Lemström, His Life and Work.'

E. BIESE: 'Verzeichniss der Publicationen des verstorbenen Professors Selim Lemström.'

Letters to Editor—L. A. BAUER: 'Work of the Department of Terrestrial Magnetism of the Carnegie Institution for 1905.' W. F. WALLIS: 'Principal Magnetic Disturbances Recorded at Cheltenham Magnetic Observatory, March 1 to May 31, 1905.'

Notes—'Additional Eclipse (August 30, 1905) Stations.' 'Miscellaneous.'

SOCIETIES AND ACADEMIES.

THE MISSOURI SOCIETY OF TEACHERS OF MATHEMATICS.

THE past few years has been a very widespread movement among teachers of mathematics towards the organization of local, state and sectional associations of teachers of mathematics. This movement is both a result and a cause of a very general dissatisfaction with methods of teaching mathematics in the recent past, and of various kinds of attempts to improve them. Among the many ideas that are prominently discussed are those suggested by the terms correlation, laboratory methods, individual instruction, self-activity, graphical methods, etc. The facts of modern life are furnishing material which is replacing obsolete problems. An effort is being made to bring mathematics into vital relations with the whole of life. Even the long undisturbed supremacy of the methods of Euclid in secondary education is being questioned. What will it lead to? Even the elementary teacher can not fail to see what the investigator has never lost sight of, that he is dealing not with a completed, a dead, a petrified subject, but with one of the most vigorous, living, growing subjects taught in our schools. Perhaps one of the strongest evidences that this is the case is seen in the large number of state and sectional organizations of teachers of mathematics throughout the country.

The first annual meeting of the Missouri Society of Teachers of Mathematics met at Columbia, Missouri, May 6, 1905. A preliminary meeting had been held at St. Louis in connection with the National Educational Association. The temporary organization of the society was effected at the meeting of the State Teachers' Association at Columbia, December 28, 1904. At a meeting of the mathematics section of that body a committee of organization was appointed, consisting of E. R. Hedrick, University of Missouri, Columbia; H. C. Harvey, State Normal School, Kirksville, and B. T. Chace, Manual Training High School, Kansas City.

The permanent organization was completed at the meeting on May 6. The constitution provides that there shall be at least two meetings each year, one in connection with the annual meeting of the State Teachers' Association, the next meeting of which will be held at Jefferson City, December 1905, and one during the month of April or May, which shall be the annual meeting for the election of officers and the transaction of general busi-

ness. The general management of the society is in the hands of an executive council of six members. Steps have already been taken towards the establishment of several divisions.

The total membership of the society is two hundred and thirty-six.

L. D. Ames, of Columbia, presided at the meeting. The following officers were elected:

President—H. C. Harvey, Kirksville.

Vice-President—L. M. Defoe, Columbia.

Secretary—L. D. Ames, Columbia.

Executive Council—E. R. Hedrick, Columbia (chairman); B. T. Chace, Kansas City; B. F. Finkel, Springfield; B. F. Johnston, Cape Girardeau; Wm. Schuyler, St. Louis; Miss E. J. Webster, Kansas City.

The monthly journal, *School Science and Mathematics,* was made the official organ of the society, and will be sent free to all members. The annual dues are one dollar and fifty cents.

Arrangements were made to send delegates to a conference to be held in connection with the National Educational Association, which met at Asbury Park, N. J., on July 7–11, 1905, looking towards the organization of a national society.

The following papers were read:

E. Y. BURTON, St. Charles Military Academy: 'Correlation of Arithmetic, Algebra, Geometry and Trigonometry.'

WM. SCHUYLER, McKinley High School, St. Louis: 'An Experiment in Individual Instruction.'

GEO. R. DEAN, School of Mines, Rolla: 'A Method of Teaching Elementary Geometry.'

J. W. WITHERS, Yeatman High School, St. Louis: 'The Teaching of Mathematics in the High School.'

F. C. TOUTON, Central High School, Kansas City: 'Some Developments in Elementary Algebra.'

WM. A. LUBY, Central High School, Kansas City: 'The Teaching of Zero and Infinity in the High School.'

Abstracts of these papers will be published in *School Science and Mathematics.*

L. D. AMES.

THE TORREY BOTANICAL CLUB.

THE meeting of May 9 was held at the New York Botanical Garden, with President Rusby in the chair and 42 members and visitors present.

The meeting was devoted to the exhibition and discussion of the various forms of American violets. The discussion was opened by Dr. N. L. Britton, who spoke of the recent specific differentiations of various authors. He was of the opinion that many of these were doubtful and that while we had, perhaps, twice as many good species as were known in Gray's time, we only have about half as many good species as have been described. The speaker then gave a general sketch of the group, noting that while they are preeminently a north temperate cosmopolitan group they extended into the southern hemisphere along the highlands in both the orient and the occident. There is only a single endemic and one introduced species known from the West Indies. Mexico furnishes, perhaps, half a dozen species, and there are numerous species in the highlands of South America. Our violets fall naturally into two habit groups, the acaulescent and the stemmed. A rather common character is the occurrence of cleistogamic flowers, which are borne on horizontal or erect scapes according to the species. The speaker passed the various species in review, paying particular attention to those of eastern North America.

Stewardson Brown, of the Philadelphia Botanical Club, was called upon to review Dr. Britton's remarks. He said that in the main he agreed with Dr. Britton's views of specific validity. He called attention to a form from the vicinity of Philadelphia which Stone recently identified as *Viola septemloba* of LeConte of the *palmata* group, and which the speaker believed to be something different. Attention was also called to *Viola obliqua,* one of the earliest and most abundant violets in the Philadelphia region. The speaker described the *sagittata-fimbriatula* group as one of the most integrated and little understood of any of the groups of acaulescent blue violets.

Continuing the discussion, W. W. Eggleston mentioned the occurrence of what he believed to be a hybrid form. He also called attention to President Brainerd's methods of studying violets under cultivation and observing their fruit characters.

L. H. Lighthipe called attention to *Viola*

Angelli, holding it to be distinct from *Viola palmata,* the differences showing in the character of the flowers and of the summer leaves. Miss Angell, who was present, told of her studies of this species and called attention to the extraordinary size of the summer leaves. Dr. Rusby in the course of his remarks mentioned a very early form which is apparently the variety *cordata* of *Viola cucullata* of Gray. This form has been studied extensively by Miss Sanial, one of the club members.

Dr. Rydberg spoke of the violets of the Rocky Mountain region, passing in review the various species from that section and calling attention to the occurrence of the common European *Viola biflora* which reappears in Colorado.

Dr. Shull spoke of the difficulty he had experienced in germinating violet seeds, and in the discussion it was brought out that violet seeds are apt to lose their vitality upon drying.

Dr. MacDougal spoke of the difficulties attendant upon mutation experiments with the violets, and advocated experiments to test any possible theories as to hybrids.

After some further discussion by Dr. Britton and others this most interesting meeting was brought to a close.

Edward W. Berry,
Secretary.

THE UNIVERSITY OF COLORADO SCIENTIFIC SOCIETY.

During the academic year 1904–5 the society met every Monday evening from October to May, holding in all thirty meetings. In nearly all cases a single topic was discussed at each session, but a few times there were two papers given. The speakers avoided technicalites as far as possible and presented their topics in such form as to be interesting to men of science generally. Papers were given, for the most part, by members of the faculty representing the various departments of pure and applied science. At the last meeting of the year, held on May 15, the following officers were elected:

President—Henry B. Dates.
Vice-President—Ira M. DeLong.
Secretary—Francis Ramaley.
Treasurer—Martin E. Miles.

Francis Ramaley,
Secretary.

Boulder, Colo.,
June 7, 1905.

SPECIAL ARTICLES.

NEW WORK UPON WHEAT RUST.

For a number of years it has been the belief of the writer that the efficiency of the uredospores (summer spores) of wheat rust to perpetuate the disease is possibly much greater than thought to be. It has been assumed by most botanists that these spores are quick to germinate and short of life. As there are formed definite resting spores, and also the cluster cup stage on the barberry bush, it has been apparently taken for granted that the summer spores have no other effect than to rapidly spread the disease from plant to plant during the summer season.

It will be interesting news to mycologists to know that we have at last definitely established the fact of the wintering of the red spores (uredospores) of a number of the important rusts in viable form, including the important species *Puccinia graminis.*

During the winter of 1888 and 1889 the writer, while working at the Indiana Station, first demonstrated the fact that the mycelium of the uredo stage (red spore stage) of the species known as *Puccinia rubigo-vera* could pass the winter in the tissues of the wheat plant uninjured (see *Agricultural Science,* Vol. 3, page 105). During the summer of 1890 (see *Agricultural Science,* Vol. 5, Nos. 11 and 12) it was further proved that the red spores of this last-named species could survive exposure to the drying air and sunshine of July and August for over a month. This indicated that it was possible for such spores to be borne many miles by the wind, and aided to an understanding of the rapidity with which general rust infection may take place over large areas of country.

Aided by the persistent and painstaking efforts of assistant plant pathologist Mr. F. J. Pritchard, I have at last been able to make numerous trial studies upon methods of stor-

ing and keeping wheat and other types of grass straw, which is infected by red rust, so as to carry out various studies upon the most successful methods of testing the vitality of the spores from week to week and from month to month. We are now able to announce definitely that the vitality of the red spores (*uredospores*) of *Puccinia graminis,* in certain cases, may remain unimpaired by the action of the drying winds of autumn and the intense cold of a North Dakota winter. In some cases we have been able to germinate as high as eighty to ninety per cent. of all the spores under test. We have found these spores successfully surviving upon dead leaves, dead straw and upon the partially dead or green leaves of living grain or grasses. This applies also to a number of other important rusts which attack wheat and allied grasses.

In the case of *Puccinia rubigo-vera,* the smaller wheat rust, it has been found by the writer to be wintering freely in Mississippi, Texas, Illinois, Minnesota and North Dakota both upon living leaves of wheat or winter rye and upon the matured leaves and straw of the same. This fact will of necessity have great weight upon the future investigations of wheat rust. The matter of the barberry stage and other æcidial rusts may yet be proved to be of physiological necessity for the perpetuation of the species, but it would seem that these need no longer be believed to be a direct yearly necessity to the perpetuation of the rusts concerned.

HENRY L. BOLLEY.
NORTH DAKOTA AGRICULTURAL COLLEGE.

CONCERNING THE IDENTITY OF THE FUNGI CAUSING AN ANTHRACNOSE OF THE SWEET-PEA AND THE BITTER-ROT OF THE APPLE.

ABOUT a year ago I received some sweet-pea stems from Inwood, W. Va., with a request as to the cause of the plants dying. These stems had dead, shrunken areas on them with masses of pink spores scattered about over the dead areas. There were also a few spore masses on some of the leaves. An examination showed that the dead areas were probably caused by some species of *Glœosporium,* but no such fungus has been found as occurring on the sweet-pea in the literature that I have had access to. I have called the disease an anthracnose on account of its resemblance to the anthracnoses of some other plants.

More material was secured at different times during the autumn, and it was my intention to make a personal investigation of the disease until after Mr. A. Lee Post became officially connected with the experiment station and a student in the university, when the problem was assigned to him under my direction. He began a study of the life history of the fungus by means of artificial cultures and inoculations. The results of the investigation, up to date, have been presented in the form of a thesis, and will probably be published later with slight alteration and the addition of new data.

While examining some of the agar cultures with Mr. Post, I noticed that there was an occasional cell of the mycelium that contained spores, the number of spores in the cells varying. To all appearances the endospores were the same as those borne externally on the hyphæ. This was the first time that I had seen endospores in the mycelium of a fungus other than those found in bacteria, and correspondence with some of the leading mycologists has failed to give me any definite light on the subject of endospore formation in the higher fungi.

The manner of growth of the mycelium and the way the conidia were produced were so characteristic of the bitter-rot fungus of the apple and the one causing the mummy disease of the guava, that Mr. Post made some inoculations in apple-agar and in apples. The result of the inoculations on apples was so similar to the bitter-rot of the apple that a number of mycologists have pronounced it genuine bitter-rot.

Through correspondence with the person who sent me the diseased sweet-pea stems, I learned that the sweet-peas grew near an apple tree, the fruit of which rotted. Just what kind of a rot it was will be determined this fall if possible. This rotting of the apples on the tree near the sweet-peas, suggested the possible identity of the anthracnose of the sweet-pea and bitter-rot of the apple. To prove

whether they are, or are not, the same, I let Mr. Post have some specimens of bitter-rot and of the ripe-rot of the grape collected at least two hundred miles from where the sweet-peas grew. Seedling sweet-peas, inoculated with spores from these two sources, were killed at the point of infection in the same way that the original sweet-pea stems were killed, and other seedlings which were inoculated with pure cultures of the fungus causing the anthracnose of the sweet-pea.

It would seem, then, from the results obtained, as if the bitter-rot of the apple, the ripe-rot of the grape and the anthracnose of the sweet-pea are caused by the same fungus. A stage corresponding to the ascigerous stage of the bitter-rot has not been obtained yet in artificial cultures.

John L. Sheldon.
West Virginia Agricultural Experiment Station, Morgantown, W. Va.,
June 19, 1905.

INDICATIONS OF AN ENTOMOPHILOUS HABIT IN TERTIARY SPECIES OF QUERCUS.

The occasional development of several embryos in the fruits of recent species of *Quercus* is of interest as suggesting an entomophilous habit in the flowers of the Tertiary species of this genus.

At present normally five of the six ovules in the three-celled ovary atrophy, and the one remaining forms later a perfect embryo which fills the entire cavity of the nut. But it not infrequently happens that two embryos develop, each with cotyledons, plumule and caulicle. Experiments made by the writer show that both embryos will grow, and the twin oaks were kept until they reached a foot or more in height. Several cases were found by the writer in which three perfect embryos occurred in acorns of the chestnut oak, *Quercus prinus*. All germinated nearly equally well. Finally a single case was found in which there were four perfect embryos. This also was an acorn of the chestnut oak, which develops several embryos more readily than *Q. alba, rubra* or *tinctoria*.

Several notes have been previously published on the development of two embryos in *Quercus*, but I have not found any record of three or of four perfect embryos occurring in this genus.

The normal abortion of five ovules and reduction to one embryo seems to be an acquired character, and in the development of several embryos appears to be a reversion to an ancestral condition.

Now, it is well known that the formation of several or many embryos is characteristic of entomophilous flowers, but very rare among anemophilous.

This suggests that the oaks of the Greenland Tertiary flora were entomophilous, that their flowers were more conspicuous, and that their fruits normally developed several embryos. With the oncoming of the ice sheet the oaks moved very slowly southward because of the inadaptability of the fruit for wide dispersal. Deserted by the insects seeking the warmth farther south, the oaks may then have adopted their present anemophilous habit.

Paleobotany so far can give no evidence either for or against this theory, but later studies of the Tertiary floras may strengthen the indication now furnished by the development of two, three and four embryos in cases of reversion in *Quercus prinus*.

C. J. Maury.

BATHYGNATHUS BOREALIS, LEIDY, AND THE PERMIAN OF PRINCE EDWARDS ISLAND.

A few days ago I had occasion to examine the figure of *Bathygnathus* published by Leidy in his original description (*Jour. Acad. Nat. Sc. Phila.* (2), 11, pp. 327–330, pl. XXXIII.) and became convinced that it was not a dinosaur, as has been long supposed, but one of the most specialized of the pelycosaurs, such as occurs in the Texas region, probably a *Dimetrodon* or *Naosaurus*. I communicated with Dr. Lambe, of the Canadian Survey, indicating my belief that this settled the question of the possible occurrence of Triassic deposits in Prince Edwards Island. Almost all of the geologists of the Canadian Survey who have worked on the island have considered the rocks as Permo-carboniferous and have

admitted the possibility of Triassic rocks in the neighborhood of New London only because *Bathygnathus* was found there and was considered as a Triassic dinosaur. Hardly had the letter been posted when I received from Dr. von Huehne his paper on the 'Pelycosaurier im Deutschen Muschelkalk' (*N. Jahrb. f. M. G. u. P. Beilage,* Band XX., p. 343), in which he arrives at exactly the same conclusion as to the nature of the fossil and the age of the beds. Aside from settling the age of the beds of Prince Edwards Island the discovery is of interest in extending the range of these forms which have previously been known from Texas, Vermillion County, Ill., and Bohemia. It is interesting also to note, as pointed out by von Huehne, that Owen in the *Q. J. G. S.*, 1876, pointed out that *Bathygnathus* was probably related to the theriodonts. This suggestion has been disregarded in favor of the dinosaurian nature of the fossil and has so kept alive the error in the age of the beds.

E. C. CASE.

A SYSTEM FOR FILING PAMPHLETS.

No system for filing pamphlets will meet the requirements of all workers, but a plan that I have used for some years has proved so satisfactory and met with the approval of so many of my friends that I venture to present a brief outline of it, in order that others may perhaps be benefited. I make no claim for originality except, perhaps, in the size of the boxes.

I use pasteboard boxes very much like those used by the Book Lovers' Library to protect its volumes, ten and one half inches high, seven and one fourth inches deep and one inch thick, the back or edge nearest the wall, as they stand on the shelf, being open.* Each box holds only a small number of pamphlets, and therein lies the chief advantage, as the small boxes facilitate a great subdivision of subjects.

In my series of 'Birds, geographic,' for instance, I have a box for faunal papers for every state in the United States and for some states several boxes, the subdivision in these cases being by authors. Every faunal bird paper is marked in the corner 'Bg,' followed by a number indicating a country, the United States being, for instance, 4, with each state designated by a decimal number, so that a Pennsylvania faunal bird list would be marked 'Bg 4.9,' the Pennsylvania box bears this label on the back and also one inscribed 'Birds of Pennsylvania.'

I have then a card catalogue of all my separata, etc., arranged by authors with a reference to the box number. It is thus possible to take from the shelf at once all the papers relative to a given subject or by the card list to locate any paper that may not be where I expected to find it or to see if I have a paper by a certain author.

In the case of a composite paper it may be arranged where most frequently sought and a cross reference be entered on a stiff sheet of octavo paper placed in the other box where it might be arranged. In fact, a sheet like this in every box with cross reference titles is of great convenience.

Bound volumes may be arranged in their proper place on the shelves and catalogued just like the pamphlets.

This system permits of endless variations in the method of classification. For my ornithological series I have the following divisions:

Ba, anatomy; Bb, bibliography; c, classification; d, destruction and extinction; e, economic ornithology; f, food; g, geographic lists; h, hybrid, albinos, etc.; l, museum catalogues; m, molt and pterylography; n, migration; o, nests and eggs; s, systematic monographs, etc.; v, song. Bg and Bs are, of course, the large series, the others occupying only two or three boxes each.

The arrangement of pamphlets relating to so broad a subject as ornithology, by authors, is almost useless, as it is impossible to remember all who have written, for instance, on the birds of Pennsylvania. My plan gives you all these papers together on the shelf without consulting the card list, while if the arrangement by authors is needed the cards furnish it.

WITMER STONE.

ACADEMY NATURAL SCIENCES,
PHILADELPHIA.

* Made by Jesse Jones Paper Box Co., 715 Commerce St., Philadelphia, at $3.50 per hundred.

CURRENT NOTES ON METEOROLOGY.

BAROMETER AND WEATHER.

Under the title 'Barometer und Wetter,' van Bebber discusses, in the *Archiv der Deutschen Seewarte* (Vol. XXVII., 1904, No. 2, pp. 1–15), the use of the barometer as a 'weather glass,' for foretelling changes of weather; refers to the studies already made to determine the barometer readings which correspond to certain weather conditions, and then investigates the relation between the readings at Hamburg and the rainfall, temperature and cloudiness, for the year, seasons and individual months during the period 1876–1900. It appears that rain falls very infrequently when the barometer is very high; that it is then extremely light, and comes only in the colder months. Rain probability shows a steady increase with decreasing pressure; at extremely high pressures (over 30.50 ins.) there is no precipitation, and at pressures below about 28.90 ins. there is always precipitation. As to temperatures, the departures are negative at the higher pressures and positive at the lower, when averaged for the year. At low pressures the departures are positive in winter and negative in summer. Cloudiness is at a minimum during highest barometric pressure, and at a maximum during low pressure. In winter, however, high pressures are usually accompanied by fog. In Central Europe most of the precipitation of the colder months comes with falling, and of the warmer months with rising pressure, while in the British Isles and over the North Sea area it comes with falling pressure in all seasons. The critical study of these relations of pressure and weather conditions, which are set forth numerically in his paper, leads van Bebber to the conclusion that a reasonably accurate judgment of existing and coming weather can be based on the readings of the barometer, especially when the location of the cyclonic and anti-cyclonic centers can be learned from the newspaper reports. The last paragraph in the article is a quotation from a publication by the same author, dated 1899, to this effect: An experience of twenty-five years has brought Dr. van Bebber to the conclusion that no reorganization of weather service work would be of any value if the present forecasts for a single day following are adhered to. These forecasts have not satisfied the agricultural interests and will not satisfy them in the future. Nor will the forecasts be satisfactory unless the general public understands better than at present the basis on which weather predictions rest.

Dr. van Bebber's work along the line of public education in weather types, and what these types means has been a most valuable one, and the present investigation, which might well be carried on for any number of American stations, is a useful extension of his previous studies.

MONTHLY WEATHER REVIEW: ANNUAL SUMMARY, 1904.

The value of back numbers of the *Monthly Weather Review* has in the past been very much decreased by the fact that an adequate index has not been prepared for each volume. The index hitherto published has been an author index, arranged alphabetically so far as the authors' names were concerned, but not arranged alphabetically so far as the titles of the papers were concerned. The result has been a large and wholly unnecessary expenditure of time in looking up some special article or note. For the year 1904, we are glad to see, there is a fairly adequate author and subject index, arranged alphabetically as a whole, although the separate articles under different subject-headings are not in all cases alphabetically arranged. Thus, for example, to take only one case, under 'Observatories' we find the following:

At Mount Tsukuba	463
At Nice	182
The results of the work done at Tegel	555
Helwan and Abbassia observatories	374
New astrophysical and meteorological	130
New mountain	131
Mount Weather Meteorological Research Observatory	601

The 1904 index is so great an improvement on the old one that we can not help hoping that the 1905 index will be still better. All those who use the *Review* will be grateful for the work of Mr. George A. Loveland, who pre-

pared the index for 1904, and will hope that he may have time next year to make a more complete index for the 1905 volume.

CLIMATE OF JERUSALEM.

PROFESSOR G. ARVANITAKIS, in the *Bulletin de l'Institut Égyptien* (4th ser., No. 49), has published a series of meteorological observations taken at Jerusalem, as well as some notes on the climate of that region. The winds from the east are extremely dry, coming as they do from the Arabian deserts. Rain comes from the western quadrants. Hail is noted as being fairly common in Palestine, and a source of injury to the fruits. The observer says that he seldom saw a heavy rainfall unaccompanied by hail. Cisterns and reservoirs supply water during the dry season of summer, and the heavy dews are very beneficial to vegetation. These dews are characteristic of Palestine, and must be seen to be fully appreciated. The climate is not described as very healthful. Dysentery, fever and rheumatism are not uncommon at Jerusalem, especially during the summer months.

MARINE METEOROLOGICAL SERVICE OF CHILE.

THE meteorological work carried on at the coast stations of Chile, from Arica in the north to the Strait of Magellan in the south, is under the direction of the so-called 'Direccion del Territorio Maritimo' of Chile. Up to the year 1899 this work was in charge of the central observatory at Santiago. An annual volume (*Anuario*) is issued, giving complete tabulations of the data for each of the eighteen stations, and including monthly and annual summaries. Thus far (1903, Vol. V., issued 1904) no discussion of these observations has been included. These littoral stations of Chile have the great advantage of varying but little in their longitude, and of being very near sea level, so that there is much uniformity in these respects. The great climatic interest of Chile, which results from its peculiar position with reference to the Cordillera of South America, and from its extraordinary contrasts in rainfall between the arid north and the rainy south, lends exceptional value to any such data as those included in the volumes here considered.

R. DEC. WARD.

NOTES ON FORESTRY.

WHY PRAIRIES ARE TREELESS.

IN a paper by Alfred Gaskill read before The Society of American Foresters, February 23, the theory that forest fires are responsible for the treeless condition of the prairies was advocated. In support of this by no means new theory, Mr. Gaskill cited some geological, physiographical, climatological and silvical facts which, in his opinion, point most emphatically to the fire origin of all true prairies. He divides the treeless area in the United States into plains and prairie. The former are treeless, primarily because of deficient moisture, and were so from time immemorial. It is different, however, with the prairies; they offer conditions favorable to tree growth and, therefore, their treeless condition presents a riddle clamoring for solution. The great prairie of the United States occupies an irregular area bounded on the east by a line that follows in a general way the ninety-fifth meridian and on the west by a line roughly extending along the ninety-seventh meridian. The eastern boundary is most irregular, its shape corroborating the fire origin of the prairie. In the north it makes a great bend eastward, enclosing half of Iowa, more than half of Illinois, and portions of Wisconsin and Indiana. Along its whole extension the prairie forms lobes and long tongues thrusting eastward into the forest. Since it is proved by the records of the Weather Bureau that the western boundary is within the limit of sufficient rainfall, capable of supporting tree growth, the whole area now occupied by the prairie is situated where forest ought to be, for there is neither lack of rain nor any condition in the soil or the vegetation that will account for the absence of trees. Mr. Gaskill assumes, therefore, that something not entirely normal caused the forest to retreat from its proper position. After a careful and detailed study of the records of forest and prairie fires in the states of Montana, Wyoming, Colorado, Texas, South

Dakota, Nebraska, Kansas, etc., he found that most of the fires occur there in the fall when the prevailing winds are from the west, and the vegetation of the plains lying to the windward of the area now prairie is exceedingly dry and combustible. The habit of the Indians of setting fire to the grass of the plains at that time of the year and its annual occurrence are matters of history. These annual fires, driven by strong westerly winds and finding no obstacle to their progress in the flat or gently rolling land, spread eastward until they reached the green timber. Year after year these fires ate their way a little farther into the forest, making the dense forest first more open and eventually entirely consuming it. The irregular projections of the prairie eastward into the forest are considered by Mr. Gaskill as the result of the work of the fires. Another evidence of its influence in causing the prairie he finds in the fact that with the settlement of the country and gradual elimination of fires the forest commenced to gain again on the prairie.

The prairies in other countries were also considered from the same point of view, and the speaker suggested that local students would probably confirm his opinion that fire has been an active agent in all such regions.

PRINCIPLES INVOLVED IN DETERMINING FOREST TYPES.

On February 23 Mr. Raphael Zon read before The Society of American Foresters a paper on forest types. These he identified as tree associations in the ecological sense, and not mere aggregations of trees as they often are conceived to be. Few will dispute the statement that the forest type is the cornerstone of silvics, and that the proper recognition of types in any forest is the first and most important question that a practical forester has to answer. "* * * The division of a forest into natural types of growth, however, is not such a simple thing as it may appear at the first glance. Stands differ from each other in many respects; they may be pure or mixed, even-aged or irregular, dense or open, of seedling or sprout origin, etc. Which of these features justifies the subdivision of the forest into types of growth, and what must we call a natural forest type? * * * When we attempt to trace to some definite causes the differences between stands composing a large forest, we finally come to two main ones: first, external physical conditions, such as climate, soil, moisture in the ground, topography, exposure, etc.; and second, interference by man, and natural accidents, such as fire, wind and so on. * * * It does not take very long to realize that segregating stands into types based on density, age or mode of origin can not be justified, since such features are not permanent and can not be characteristic of any definite forest type. * * * A forester who mistakes any such temporary forest growth for the original natural types of growth, thus failing to understand the natural evolution of the forest, will always have nature against him in all his operations, instead of being aided by her. * * * The physical conditions of the situation then are the main factors which determine the whole character of a forest type. Of these physical factors, climate undoubtedly has a marked influence upon plant life, if we compare vegetation of different latitudes. * * * Soil, moisture in the ground, and topography, to which in mountain countries must be added altitude and exposure, are the main factors which determine the character of forest growth in a forest region, and, therefore, must be accepted as the basis for the division of the forest into natural types of growth. A natural forest type then is an aggregation of stands which may differ from each other in age, density and other secondary features, but have the same physical conditions of situation, like soil, topography, exposure, etc. * * * The relationship between the physical conditions of the situation and the character of growth upon it is so constant and characteristic that by the given physical conditions of a situation, like soil, topography, and so on, one can describe the general character of its forest, the predominant species, habit of trees, reproduction, undergrowth and, *vice versa,* by a given type of the forest growth one can describe the physical conditions of growth, soil, situation, etc. * * * A forest

type is the result of a long struggle for existence between different species, in which only those possessing the greatest vitality and best fitted to the physical conditions of situation succeed in occupying the ground and form tree associations having a distinct physiognomy. One of the most important characteristics of a forest type is its stability, its resistance to invasion by other plant forms. * * *"

THE FOSSIL ARACHNIDA OF BOHEMIA.

WE are indebted to Professor Dr. Anton Fritsch for another important contribution on the Permian and Cretaceous fauna of Bohemia entitled 'Neue Fische und Reptilien.' This takes the form of a quarto appendix to his previously published volumes, and is illustrated by nine plates. The Cretaceous forms described are new teleosts, plesiosaurs, mosasaurs and pterosaurs.

In 1904 there appeared from the pen and brush of this ardent paleontologist a fine monograph on the Paleozoic arachnida, consisting of eighty pages of text and fifteen plates. The conclusions reached in this monograph are most striking, especially as to the very great antiquity of modern forms. The author observes "If we examine the entire series of the forms described we must recognize that there are many which present no very striking differences from the Arachnida of to-day. They are to be regarded as the direct ancestors of families now existing in part as lateral branches which have later become extinct." This is true of members of six families described. The scorpions of the Silurian period show in their foot structure a primitive form suggesting that of the Crustacea whereas those of the Carboniferous and Permian formations exhibit close resemblance to the foot structure of the modern types.

H. F. O.

EXTENDED EXPLORATIONS OF THE ATMOSPHERE BY THE BLUE HILL OBSERVATORY.

ACCOUNTS of the first experiments in this country with *ballons-sondes,* for the purpose of ascertaining the meteorological conditions at great heights above the American continent, appeared in SCIENCE, Vol. XXI., pp. 76–77 and 335. During the months of January, February and March, 1905, nine more ascents were made from St. Louis and every balloon but one was found and, with the attached instrument, was returned to Blue Hill in accordance with the instructions on each. Like the previous balloons, all of these fell within the eastern half of a circle having its center at St. Louis and a radius of 285 miles. The German expanding rubber-balloons, filled with hydrogen generated by the vitriolic process, were again employed, as were the French self-recording instruments, which gave at least partial records of barometric pressure and air-temperature in seven of the nine ascensions, although another record was obliterated by the finder. On January 25, when a high barometric pressure prevailed at the ground, a temperature of —111° F. was recorded at the height of 48,700 feet, this being one of the lowest natural temperatures ever observed. The experiments last winter were conducted by Mr. Clayton, under the direction of Mr. Rotch, and their success induced Professor Langley, secretary of the Smithsonian Institution, to grant Mr. Rotch $1,000 from the Hodgkins Fund, in order to continue the experiments this summer at St. Louis. These, like the first, will be conducted by Mr. Fergusson, of the Blue Hill Observatory staff. Soundings of the atmosphere made at different seasons should reveal the annual variation of temperature at great heights above the American continent, which is at present unknown.

However, kites are not neglected at Blue Hill, for, besides the flights made there each month on the days fixed by an international committee, they are also being employed to ascertain the conditions above the Atlantic Ocean in the trade-wind region. Thus the investigation which was first proposed by Mr. Rotch in SCIENCE, Vol. XIV., pp. 412–413, and which has been persistently advocated by him since, is now in progress, and this was rendered possible through the cooperation of the well-known French meteorologist, M. L. Teisserenc de Bort, who placed his steam-yacht at the disposal of Mr. Rotch, on condi-

tion that the latter should share the expense of the cruise. Accordingly, on June 3, Mr. Clayton sailed from Boston for the Mediterranean on board the White Star steamer *Romanic*, equipped for raising self-recording instruments with kites, as was first done in 1901 by Messrs. Rotch and Sweetland, whose experiments on a voyage from Boston to Liverpool are described in SCIENCE, Vol. XIV., pp. 896–897. A despatch from Mr. Clayton, at Gibraltar, announced that flights had been made on six days and a mean height of 3,000 feet attained. The results of aerial soundings in the region of permanent high pressure around the Azores, and near the northern limit of the northeast trades, are expected to prove of special interest. At Gibraltar, Mr. Clayton is to embark on the *Otaria*, a yacht of 350 tons and capable of steaming eleven knots, which its owner has already employed for kite-flying in European inland waters. The boat will proceed south, touching at Madeira, Canary and Cape Verde Islands, and perhaps go as far as St. Paul, near the equator, returning by a more westerly course to the Azores, the whole voyage occupying about six weeks. On this route the northeast trade-winds and doldrums are traversed and the southeast trades entered. Should there be too little wind, either at the surface or higher up, the speed of the vessel will enable the kites to rise, and, should the wind at any time be too strong, by steaming with it the pull of the kites can be moderated. By this method it is hoped that all the strata up to a height of 15,000 feet or more will be penetrated, so that their condition as regards temperature, moisture and wind may be investigated. Besides determining the depth of the northeast trade-wind, the supposed southwest, or return trade, which has only been observed on the Peak of Teneriffe, will be sought and its height above the ocean in different latitudes measured, but in case the kites do not reach a sufficient altitude, it is proposed to liberate small balloons from Madeira and observe their change of direction as they rise. Professor Hergesell, on board the Prince of Monaco's yacht, executed last summer a series of kite-flights in the region between Spain, the Canaries and the Azores, without encountering the upper anti-trade, as was mentioned in SCIENCE, Vol. XXI., p. 464. The present expedition expects to make similar soundings in these and lower latitudes and will attempt to extend them to greater heights.

REGULATIONS GOVERNING THE SIXTH INTERNATIONAL CONGRESS OF APPLIED CHEMISTRY.

THE sixth International Congress of Applied Chemistry, under the patronage of His Majesty the King of Italy, will be held at Rome in the spring of 1906. The president of the committee of organization is Professor Emanuele Paterno, Via Panisperna, Rome, and the secretary, Professor Vittorio Villavecchia, Central Customs Laboratory, Rome. All who are interested in promoting the applications of chemistry are eligible for membership in the congress. Active members are those who signify their desire to be such to the general secretary either before the opening of the congress or during its session, and who send the subscription fee, twenty francs. Donating members comprise those persons or societies who contribute the sum of at least one hundred francs or lire. Those who give a thousand lire or more belong to the list of patrons.

The congress is divided into the following sections and subsections:

1. Analytical chemistry, apparatus and instruments—president, Pietro Spica, professor of pharmaceutical chemistry in the University of Padua.

2. Inorganic chemistry and industries related thereto—president, Luigi Gabba, professor of technical chemistry in the Higher Technical School, Milan.

3. Metallurgy and mining, explosives; (*a*) metallurgy and mining—president, Ettore Mattirolo, Geological Survey, Rome; (*b*) explosives—president, Cav. Guiseppe Ninci, colonel of artillery, superintendent of powder of Fontana Liri.

4. Organic chemistry and industries related thereto; (*a*) industry of the organic products —president, Giacomo Ciamician, professor of general chemistry in the University of Bo-

logna; (*b*) coloring matters and their uses—president, Professor Guglielmo Koerner, director of the Higher School of Agriculture, Milan.

5. Technology and chemistry of sugar—president, Professor Vittorio Villavecchia, director of the Customs Laboratory, Rome.

6. Fermentation and starch; (*a*) fermentation, with special regard to enology—president, Mario Zecchini, director of the experiment station at Turin; (*b*) industry of starch and its derivatives—president, Professor Giglioli Italo, director of the experiment station of Rome.

7. Agricultural chemistry—president, Angelo Menozzi, professor of general chemistry in the Royal School of Agriculture, Milan.

8. Hygiene; (*a*) hygiene and medical chemistry—president, Icilio Guareschi, professor of pharmaceutical chemistry in the University of Turin; (*b*) pharmaceutical chemistry—president, Luigi Balbiano, professor of pharmaceutical chemistry in the University of Rome; (*c*) bromatology—president, Arnaldo Piutti, professor of pharmaceutical chemistry in the University of Naples.

9. Photochemistry, photography—president, Colonel Giuseppe Pizzighelli, president of the Italian Photography Society, Florence.

10. Electro-chemistry, physico-chemistry—president, Raffaello Nasini, professor of general chemistry in the University of Padua.

11. Laws, political economy and legislation in relation to industrial chemistry—president, G. B. Pirelli, Ponte Seveso 16, Milan.

The languages used in the discussion are Italian, French, German and English. The minutes of the proceedings of the session will be in Italian.

Those who intend to present papers and communications to the congress should send them to the general secretary at least two weeks before the opening of the congress. They should be written in one of the official languages of the congress, and as far as possible should be brief and succinct.

The congress shall consist of general meetings and meetings of the separate sections. There shall be at least two general meetings, of which the dates will be fixed by the committee. The sections may hold any number of meetings, and will work independently of each other.

The first general session shall be presided over by the chairman of the organizing committee. At this meeting the officers shall be elected, and shall include: an honorary president, and acting president, acting vice-president and honorary vice-presidents, a general secretary and assistant secretaries. In the last meeting of the congress at large shall be fixed the date and the place for the seventh congress of applied chemistry.

A president chosen by the nominating committee shall preside over the first meetings of the sections and subsections. In that meeting shall be elected a president, one or more vice-presidents, a secretary and one or more assistant secretaries. At the end of each meeting the president of the succeeding meeting shall be elected by vote. The president chosen by the nominating committee shall be a member of the board of the section in all its meetings.

The sections can modify their order from day to day. Actions shall be determined by a majority vote—in case of a tie the vote of the president shall decide.

The presentation of a topic shall not last more than twenty minutes, and during the discussion, speakers shall not occupy the floor for more than five minutes, nor speak more than twice on the same question, without special permission from the chair.

The minutes of a meeting shall include: (*a*) a summary of the papers presented, in the order in which they were given, the names of the speakers, and a summary of the ensuing discussion; (*b*) the decisions adopted in each meeting.

To make possible the accurate reproduction of the transactions of each meeting, the members who have spoken will send to the secretary of the section, at the latest, half an hour after the close of the meeting, a brief summary of the arguments of their papers. This summary shall be written in one of the four official languages. The secretary shall arrange the transactions, and transmit them, together with a list of the members present,

before twenty hours, to the general secretary of the congress.

Decisions of importance and international character adopted by the congress will be communicated to the governments participating in the International Committee of the Congress of Applied Chemistry, established at Paris in 1900.

After the close of the congress, a report will be printed containing the papers presented and the decisions adopted by the congress. This report will be sent free of charge to members.

All questions not provided for in this list of regulations shall be decided by the president, who shall continue in authority without cessation on matters pertaining to the present congress. Before he retires he shall place the conduct of affairs in the hands of an organizing committee for the next congress.

Dr. H. W. Wiley, at the request of the committee on organization, undertook the organization of an American committee, which is composed of members selected by President F. P. Venable on the part of the American Chemical Society, President R. W. Moore on the part of the New York section of the Society of Chemical Industry, and President H. S. Carhart on the part of the American Electro-Chemical Society.

Intending congressists may send their names and dues directly to the secretary of the congress, together with titles of papers to be presented at the meeting, or if preferred, to the chairman of the American committee, who will undertake to forward names, dues and papers to Rome. In case the dues are sent first to Washington for transmission to Rome, a check for $4.00 should be sent to cover dues, exchange and postage.

LIST OF MEMBERS OF THE AMERICAN COMMITTEE. SIXTH INTERNATIONAL CONGRESS OF APPLIED CHEMISTRY.

Nominated by the Society of Chemical Industry (New York Section) and the American Chemical Society.

Dr. Charles Baskerville, chemist, College of the City of New York, New York.

Dr. Edward Hart, professor of chemistry, Lafayette College, Easton, Pa.

Dr. W. D. Horne, chemist, The New York Refinery, Long Island City, N. Y.

Dr. Leo Baekeland, research chemist, Snug Rock, Harmony Park, Yonkers, N. Y.

C. F. Chandler, professor of chemistry, Columbia University, New York, N. Y.

Nominated by the Electrochemical Society and the American Chemical Society.

W. D. Bancroft, professor of physical chemistry, Cornell University, Ithaca, N. Y.

C. F. Burgess, Madison, Wis.

Nominated by the Society of Chemical Industry (New York Section) and the American Electrochemical Society.

Dr. W. H. Walker, 93 Broad Street, Boston, Mass.

Nominated by the American Chemical Society.

Wm. A. Noyes, chemist, National Bureau of Standards, Washington, D. C.

J. D. Pennock, chief chemist, Solvay Process Co. and Senet-Solvay Co., Syracuse, N. Y.

Dr. Chas. E. Munroe, professor of chemistry, George Washington University, Washington, D. C.

Francis C. Phillips, professor of chemistry, Western University, Allegheny, Pa.

William McMurtrie, consulting chemist, Royal Baking Powder Co., New York, N. Y.

Dr. J. Merritt Matthews, Philadelphia Textile School, Philadelphia, Pa.

Mr. Clifford Richardson, director, New York Testing Laboratory, Long Island City, N. Y.

Dr. Samuel P. Sadtler, consulting chemist, Franklin Institute of Philadelphia, Philadelphia, Pa.

Dr. F. G. Wiechmann, consulting chemist, American Sugar Refining Co., Box 79, Station W, Brooklyn, N. Y.

David L. Davoll, Jr., chief chemist, Peninsular Sugar Refining Co., Caro, Mich.

Mr. G. L. Spencer, Bureau of Chemistry, Washington, D. C.

Dr. Max Henius, director, American Brewing Academy and the Scientific Station for Chicago, Chicago, Ill.

Charles E. Pellew, adjunct professor of chemistry, Columbia University, New York, N. Y.

Dr. Alfred Springer, chemist, 312 E. 2d Street, Cincinnati, Ohio.

Mr. B. W. Kilgore, director, North Carolina Agricultural Experiment Station, and state chemist, Raleigh, N. C.

Dr. Henry Adam Weber, professor agricultural chemistry, Ohio State University, Columbus, Ohio.

Chas. D. Woods, professor of agriculture, Uni-

versity of Maine, and director of Maine Agricultural Experiment Station, Orono, Me.

B. B. Ross, professor of chemistry, Alabama Polytechnic Institute, Auburn, Ala.

M. E. Jaffa, professor of chemistry, University of California, Berkeley, Cal.

Edward Kremers, professor of pharmaceutical chemistry, University of Wisconsin, Madison, Wis.

John Marshall, professor of chemistry, University of Pennsylvania, Philadelphia, Pa.

W. P. Mason, professor of chemistry, Rensselaer Polytechnic Institute, Troy, N. Y.

Dr. Chas. E. Doremus, assistant professor of chemistry, College of the City of New York, N. Y.

Edgar F. Smith, professor of chemistry, University of Pennsylvania, Philadelphia, Pa.

Dr. J. W. Mallet, professor of chemistry, University of Virginia, Charlottesville, Va.

Chas. B. Dudley, chief chemist, Pennsylvania R. R. Co., Altoona, Pa.

Mr. A. M. Todd, manufacturing chemist and distiller of essential oils, Kalamazoo, Mich.

Dr. Hugo Schweitzer, 40 Stone Street, New York, N. Y.

Mr. Maximilian Toch, 468 West Broadway, New York, N. Y.

Mr. Geo. C. Stone, chief engineer, New Jersey Zinc Co., 11 Broadway, New York, N. Y.

Dr. Theodore B. Wagner, The Rookery, Chicago, Ill.

Dr. William H. Parker, Office of the Appraiser, Boston, Mass.

E. F. Roeber, 114 Liberty Street, New York, N. Y.

Dr. J. W. Richards, Lehigh University, Bethlehem, Pa.

Mr. Hugh Rodman, 52–56 St. Clair Street, Cleveland, O.

Mr. W. McA. Johnson, care St. Nicholas Club, 7 West 44 St., New York, N. Y.

Mr. Henry Noel Potter, 510 West 23d Street, New York, N. Y.

Professor S. P. Sharples, Broad Street, Boston, Mass.

Dr. John A. Mandel, Bellevue Medical College, New York, N. Y.

ARTHUR WOODBURY EDSON.

ON Friday, June 23, Mr. Arthur Woodbury Edson, of the Bureau of Plant Industry, United States Department of Agriculture, died suddenly at Waco, Texas. The news of Mr. Edson's death came as a complete surprise and shock to his colleagues and friends in the department. The members of the Bureau of Plant Industry who had been closely associated with him held an informal meeting on Saturday afternoon, June 24, and passed resolutions of condolence and sympathy for his bereaved wife and parents. Dr. Erwin F. Smith, Dr. H. J. Webber and others gave expression to their regard for the man and their admiration of his scientific work.

Mr. Edson received his training in botany and in agricultural science at the University of Vermont, studying under Professor L. R. Jones and receiving the degrees of bachelor and master of science. Professor Jones early discovered his great promise, as he showed from the first great aptitude and enthusiasm for scientific work. While at Vermont he devoted a great deal of attention to the problems connected with the sap flow of the sugar maple. The bulletin upon that subject published a year ago by the Vermont Agricultural Experiment Station is based largely upon Mr. Edson's studies and experiments.

On July 1, 1901, Mr. Edson was appointed a scientific aid in the Bureau of Plant Industry, Department of Agriculture, entering at once upon plant breeding work, and assisting Dr. H. J. Webber in the breeding of long-staple upland cottons in South Carolina. Subsequently, Mr. Edson was promoted to the position of assistant physiologist, and was given charge of the plant breeding work upon cotton in Texas and adjacent states. When the plant breeding laboratory was called upon to cooperate with other organizations in the department in the cotton boll weevil investigations, Mr. Edson was entrusted with the direction of that part of the work. At the time of his death he had made great progress in producing not only earlier maturing varieties of cotton which escape the worst ravages of the pest, but varieties adapted to conditions in Texas having other desirable characters, such as greater yield, longer staple, larger bolls and easier picking qualities. Much as he had already accomplished along this line, he gave promise of even greater achievements, had not his untimely death cut short his work.

In view of Mr. Edson's many valuable qualities as an earnest and capable investigator, as

well as of the great importance and difficulty of the problem that had been put into his hands, the Department of Agriculture has sustained a very heavy loss in his death. His fellow workers feel that they have lost a more than usually upright and conscientious associate and a true-hearted, unselfish friend.

GEORGE HOMANS ELDRIDGE.

We regret to record the death of Mr. Geo. H. Eldridge, geologist of the Geological Survey, which occurred on June 29. The Washington *Evening Star* says of Mr. Eldridge: "Born in Yarmouth, Mass., on Christmas day, 1854, he gained his early education at the Boston Latin School, where he took high rank in scholarship and in military affairs. Entering Harvard College with the class of '76, he devoted special attention to geological studies, and was taken on the 'varsity glee club. After graduation he taught until selected by Raphael Pumpelly as special expert on coal and base metals for the tenth census. At the conclusion of that investigation he was placed in charge of responsible work as geologist of the northern transcontinental survey and rendered valuable service in determining the extent and value of the coals of the Rocky Mountain region. In 1884 he entered the United States Geological Survey. For several years thereafter he was engaged in field work in several western states and Florida. In 1898 he was in charge of geological investigations in Alaska, and in recent years has been occupied almost entirely in making extended studies of special mineral deposits. Mr. Eldridge won an international reputation as an expert on asphalt, was an authority on phosphate and coal, and when his last sickness overtook him was engaged in preparing an exhaustive report on the oil of California. He has contributed valuable knowledge to the science of geology and has published 'Report on Montana Coal Fields,' 'Industries of the Base Metals,' an important portion of a monograph on 'Geology of the Denver Basin of Colorado,' 'Report on Asphalt and Bituminous Rock Products of the United States,' and numerous papers on various mineral deposits in this country and Alaska. He was a member of the American Institute of Mining Engineers, the Geological Society of America and the Geological, the Anthropological and the Harvard Societies of Washington.

SCIENTIFIC NOTES AND NEWS.

Dr. E. Ray Lankester, director of the British Museum of Natural History, has been elected president of the British Association for the meeting to be held next year at York.

The Rev. Dr. Nathan C. Schaeffer, Pennsylvania state superintendent of public instruction, has been elected president of the National Educational Association.

Oberlin College has conferred the degree of D.Sc. on the Rev. John C. Gulick, known for his work on the shells of the Hawaiian Islands and his theories regarding segregation and isolation as factors in organic evolution.

Amherst College has conferred its LL.D. on President Carroll D. Wright, of Clark College, and its D.Sc. on Mr. Henry Noel Potter, an electrical engineer of New York City, and on Dr. J. F. McGregory, professor of chemistry at Colgate University.

Hobart College has conferred the degree of doctor of science on Dr. James M. Coit, teacher of natural science in St. Paul's school.

The University of Dublin has conferred its doctorate of science on Professor E. A. Schäfer, professor of physiology at Edinburgh University, and on Professor Sydney Young, professor of chemistry, at University College, Dublin.

Mr. T. A. Rickard has resigned the editorship of the *Engineering and Mining Journal*, his resignation having taken effect on June 30.

Dr. Harvey W. Wiley, chief of the Bureau of Chemistry, of the Department of Agriculture, has gone to Europe under instructions from Secretary Wilson to make an exhaustive investigation of methods of preparing and preserving food products for shipment to America.

The United States cruiser *Minneapolis*, conveying Rear Admiral Colby M. Chester, superintendent of the United States Naval Observatory, and the other members of the

government expedition which will observe the eclipse of the sun at Bona, Algeria, and Valencia, Spain, on August 29, has sailed for Spain. The auxiliary cruiser *Dixie* and the collier *Cæsar,* which have on board the instruments and materials for the observation stations, are also on their way to the Mediterranean.

IT appears from the daily papers that Professor Robert Koch, who is at present in German East Africa, has made important discoveries in regard to the transmission of tropical diseases.

M. ELIE METCHNIKOFF, vice-director of the Pasteur Institute, Paris, has been made an associate of the Belgian Academy of Sciences.

THE Guy silver medal of the Royal Statistical Society has been awarded to Mr. R. Henry Rew for his work on the production and consumption of meat and milk.

DR. ALBERT ORTH, professor of agriculture in the Berlin School of Agriculture, celebrated, on June 15, his seventieth birthday.

DR. E. WARBURG, president of the Reichsanstalt, has been appointed honorary professor in the University of Berlin.

DR. F. SIEGLBAUER, of Prague, has been appointed curator of the Anatomical Institute at Leipzig.

PROFESSOR J. VOLNEY LEWIS, of Rutgers College, will devote the summer to a special investigation of the petrography of the Newark (Triassic) traps of New Jersey and their associated copper ores for the State Geological Survey.

WE learn from *Nature* that the civil list pensions granted during the year ended March 31, include the following: 1904, August 8.—Mr. W. F. Denning, in consideration of his services to the science of astronomy, £150. August 8.—Miss Elizabeth Parker, in recognition of the services rendered to science as an investigator by her late father, Mr. W. Kitchen Parker, F.R.S., £100. August 8.—Lady Le Neve Foster, in consideration of the services rendered to mining science by her late husband, Sir Clement Le Neve Foster, F.R.S., and of the fact that his death was due to the effects of poisoning by carbonic oxide gas while carrying out his official duties, £100. 1905, January 17.—Dr. J. G. Frazer, in recognition of his literary merits and of his anthropological studies, £200. March 22.—The Rev. Lorimer Fison, in recognition of the originality and importance of his researches in Australian and Fijian ethnology, £150. March 22.—Dr. W. Cramond, in consideration of his antiquarian researches, more particularly in connection with the ecclesiastical and burghal history of Scotland, £80. March 22.—Miss L. C. Watts and Miss E. S. Watts, in recognition of the services of their late father, Mr. Henry Watts, to chemistry, £75.

THE Paris Aeronautic Club will erect in Paris a statue in memory of Philippe Lebon, the discoverer of illuminating gas.

A BUST of Michel Faraday by Mr. H. C. Fehr, presented to the Borough Polytechnic Institute, London, by Mr. Passmore Edwards, was unveiled on June 28 by Professor Sylvanus P. Thompson.

THE Berlin municipality has appropriated $20,000 to erect a statue in honor of Rudolf Virchow, which will be placed on the Karlsplatz, close to the Charity Hospital.

DR. WM. THOS. BLANDFORD, F.R.S., the well-known British geologist, died on June 23, at the age of seventy-three years. Mr. Blandford served on the Geological Survey of India from 1855 to 1882. He accompanied the Abyssinian expedition as geologist in 1868 and published on his return his 'Observations on the Geology and Zoology of Abyssinia.' He was also a member of the Persian Boundary Commission of 1872, and published a volume on the geology of eastern Persia. Dr. Blandford was a president of the Asiatic Society of Bengal from 1878 to 1879; president of the Geological Section of the British Association in 1884; president of the Geological Society of London from 1888 to 1890, and vice-president of the Royal Society from 1892 to 1893 and from 1901 to 1903. He was the recipient of the Abyssinian medal, the Royal medal of the Royal Society and the Wollaston medal of the Geological Society. He was the editor of 'Fauna of British India,' to which

he contributed one volume on mammals and two volumes on birds.

Mr. James Mansergh, F.R.S., a British engineer, well-known for his work on water-supply and sewage, died on June 15.

The government of the Argentine Republic has established a Meteorological Observatory on New Year's Island, which is expected to furnish valuable data from the South Atlantic Ocean.

We learn from *The Athenæum* that Professor Karl Schröter and his pupil Dr. Rubel have established a biological station on the Bernina Pass. Although it is intended chiefly for the study of Alpine flora, attention will also be paid to meteorological observations, and the station is supplied with a complete equipment of meteorological and geodetic instruments. Professor Schröter's present idea is to keep the station open during the whole year, and similar stations are to be established in Puschlav and in the Upper Engadine.

We learn from *Nature* that the Stephen Ralli memorial—a laboratory for clinical and pathological research—was opened at the Sussex County Hospital, Brighton, on June 29.

The New York Board of Estimate has appropriated $17,000 towards exterminating the mosquitoes on Staten Island.

UNIVERSITY AND EDUCATIONAL NEWS.

The board of regents has authorized the construction of a wing of the museum of the University of Nebraska.

The tuition fees in Sibley College and the College of Civil Engineering at Cornell University have been increased from $125 to $150. A matriculation fee of $5 will be required of all students and the fees for graduation have been increased.

Professor E. J. Wickson has been appointed acting director of the Agricultural Experiment Station of the University of California during the leave of absence granted to Professor E. W. Hilgard.

Dr. Karl E. Guthe, of the U. S. Bureau of Standards, formerly assistant professor of physics in the University of Michigan, has been appointed professor and head of the department of physics in the State University of Iowa.

Dr. Edward Bartow, associate professor of chemistry, at the University of Kansas, has been appointed associate professor of chemistry in the University of Illinois. He will have charge of the state water survey, and other sanitary work in the state.

Professor Samuel Avery, who holds the chair of agricultural chemistry at the University of Nebraska, has been appointed professor of chemistry and director of the chemical laboratory, in succession to Professor H. H. Nicholson, who has resigned. In the same institution Mr. J. H. Powers has been appointed instructor in zoology.

The regents of the Kansas State University have made the following appointments: P. F. Walker, of the University of Maine, to be associate professor of mechanical engineering; H. A. Rice, of Lehigh, assistant professor of civil engineering; N. J. Wheeler, of Purdue University, assistant professor of civil engineering; Dr. M. F. Sudler, of Johns Hopkins and Cornell Universities, assistant professor of anatomy.

Mr. G. E. Condra has been promoted to an assistant professorship of geography and economic geology in the University of Nebraska.

Mr. George D. Hubbard, instructor in geology and physical geography in Cornell University, has been elected assistant professor of geology in Ohio State University. The other members of the geological department are Charles S. Prosser, professor of geology, and John A. Bownocker, professor of inorganic geology.

The Paris Academy of Sciences has nominated M. Verneuil as its first choice for the chair of ceramics, and M. Rosenstiehl for the chair of dyeing, in the National Conservatory of Arts and Measures.

Dr. Ernst Meumann, of Zurich, has been called to the chair of philosophy at Königsberg.

Dr. Ritchie, reader in pathology at Oxford, has been made professor.